Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

most stringent system which popular opinion can be made to tolerate, not fifty per cent of the personal property subject under law to assessment ever comes within the cognizance of the assessors.

DAVID A. WELLS.

---

## ART. VI.—CRITICAL NOTICES.

1.—*Sound.* By JOHN TYNDALL, D. C. L., LL. D., F. R. S., Professor of Natural Philosophy in the Royal Institution of Great Britain. Third Edition. London: Longmans, Green, & Co. 1875.

THE chief differences between this new edition of a popular and excellent text-book and the former issues consist in the insertion, before the last two chapters, of a new chapter (numbered VII.), of sixty-seven pages, entitled "Researches on the Acoustic Transparency of the Atmosphere in Relation to the Question of Fog-Signalling"; and in a Preface of nineteen pages, mainly occupied in combating the views promulgated by Professor Henry in the Appendix to the last U. S. Lighthouse Board Report, for 1874.

It is well known, from various publications, that Professor Tyndall was engaged under the auspices of the Trinity House, during a great portion of the year 1873 (beginning in May), in investigating the subject of fog-signals,—a subject which had been under investigation by the U. S. Lighthouse Board from a much earlier date and for a much longer period; and Major Elliot, in his recent valuable Report of observations on European Lighthouse Systems, was struck with the fact that "our shore fog-signals are vastly superior, both in number and power," to those of England or of Europe.

Notwithstanding that the results of acoustic observations made from time to time by Professor Henry and his coadjutors during the past ten years, and communicated to the Lighthouse Board, must have been known to its Engineer Secretary,—Major Elliot,—he has occupied one eighth of the Report alluded to, with the detail of Professor Tyndall's observations, mainly embracing the same results; being struck probably with the differences of explanation offered on some of the observed phenomena. The natural tendency, however, of such extended references to foreign investigations, in a narrative addressed to the U. S. Lighthouse Board by its own agent, is obviously as its chairman has stated in the official Report of 1874, "to convey the idea that the facts which it states were new to the Board, and that the latter had obtained no results of a similar kind; while a reference to the Appendix to this Report will show that the researches of our Lighthouse Board have been much more extensive on this subject than those of the Trinity House, and that the latter has established no facts of practical importance which had not previously been observed and used by the former."

This passage is quoted by Professor Tyndall, and he remarks upon it (Preface, p. 11, foot-note): "It will be borne in mind that the Washington Appendix was published nearly a year after my Report to the Trinity House." While this may be accepted as an evidence of the originality of said Report, it does not establish priority of its subject, since corresponding Reports were made to the U. S. Lighthouse Board in 1855, by Professor J. H. Alexander; in 1865, 1867, and in recent years, by Professer Henry; and in 1871, by General Duane. Professor Tyndall, however, does not appear to be quite satisfied with the broad statement of Professor Henry. He says: "My desire is to be strictly just; and this desire compels me to express the opinion that their Report fails to establish the inordinate claim made in its first paragraph. It contains observations, but contradictory observations; while as regards the establishment of any principle which should reconcile the conflicting results, it leaves our condition unimproved." (*Preface*, p. 15.)

That the observations were "contradictory" is one of their merits, as the essential prerequisite to "the establishment of any principle" that should have a permanent value; and this merit is shared by the observations of Professor Tyndall, who records that "the most conflicting results were at first obtained" (p. 321). The failure to apprehend the complexity of the conditions controlling the propagation of sound, leading to hasty but natural generalizations from a single well-marked occurrence, or from a small number, has tended to impair the confidence of subsequent investigators in the accuracy of previous "contradictory observations."

There is no doubt that were the striking experiment made by Delaroche in 1816, to be repeated under the same essential circumstances, it would be found that a particular sound is carried farther at right angles to the direction of the wind than in its direction. But this is certainly not the general fact, though sometimes so stated.* And the "contradictory observations" as to the effect of wind upon sound generally have led some persons to deny its influence altogether. Thus General Duane in his instructive Report to the U. S. Lighthouse Board, January 12, 1872, detailing the results of experiments made at Portland in 1871, concludes that "the force and direction of the wind have much less influence than has generally been supposed," since he had previously noticed that while the signal sound is often heard at a much greater distance in one direction than in another, "this is not the effect of wind, as the signal is frequently heard much farther against the wind than with it."

---

* "The strangeness of Delaroche's results consisted in his establishing by quantitative measurements, not only that sound has a greater range in the direction

On this subject there is in the researches of Professor Tyndall a singular lack of any original observation or suggestion; and as to the true character of the perturbations of sound by aerial currents, he "leaves our condition unimproved." In Chapter VII. Section 3, "Contradictory Results," he says: "None of the meteorological agents observed could be singled out as the cause of these fluctuations. The wind exerts an acknowledged power over sound, but it could not account for these phenomena. On the 25th of June, for example, when the range was only six and a half miles, the wind was favorable; on the 26th, when the range exceeded nine and one fourth miles, it was opposed to the sound" (p. 268). Not a thought occurs here that aerial currents are seldom uniform for any great extent; not a suspicion that, above the observer's head, the wind might differ either in direction or in force, or, if it did, that either of these circumstances could in any way affect the radiations of sound; no idea of differences of temperature in atmospheric strata exercising any action of acoustic refraction; but only the most cursory notice is taken that "the wind was favorable," or "was opposed to the sound."

The most significant remark in his work, on the "Action of Wind" is the following from Chapter VII. Section 7: "On the leeward side of the Foreland, on the 23d of October, the sounds were heard at least four times as far as on the windward side. . . . . This well-known effect of the wind is exceedingly difficult to explain. Indeed, the only explanation worthy of the name is one offered by Professor Stokes. . . . . In a short, but exceedingly able communication presented to the British Association in 1857, the eminent physicist above mentioned points out a cause which, *if sufficient*, would account for the results referred to." (pp. 309, 310, 311.)

---

of the wind than in the opposite direction, but that the range at right angles to the wind is the maximum." (*Sound*, pp. 310, 311.) Professor Tyndall adds in a footnote (p. 311): "Experiments so important as those of Delaroche ought not to be left without verification. I have made arrangements with a view to this object."

To Professor Henry belongs the credit of having first established the principles reconciling the "contradictory observations" made by himself and by others, and demonstrating by a carefully discriminated series of facts the "sufficiency" of the theory proposed by Professor Stokes.

During Professor Tyndall's visit to this country, he was present December 11, 1872, at the reading of a paper by Professor Henry, "On certain abnormal Phenomena of Sound in Connection with Fog-Signals," before the Philosophical Society of Washington. (Bulletin, Phil. Soc. W., Vol. I. p. 65.) Almost immediately on his return to England, in 1873, he began a series of investigations on the same subject. "On the 19th of May, 1873, this inquiry began. The South Foreland, near Dover, was chosen as the signal station." (*Sound*, p. 260.) Yet throughout the numerous channels of publication in which his results were spread, one looks in vain for an allusion to any similar work having been done in this country.

In the new Preface (dated June, 1875), after noticing somewhat superfluously, though with an air of impartial commendation, the experiments of Professor Henry, published in the Lighthouse Board Report, on the character of the instruments employed as fog-signals (*Preface*, pp. 12, 13), he adds vaguely: "I was quite aware, in a general way, that labors like those now for the first time made public had been conducted in the United States, and this knowledge was not without influence upon my conduct." (p. 14.) What acknowledged influence these earlier labors of Professor Henry in the same field had does not appear from anything to be found in Professor Tyndall's writings. That the Preface in its present form is due to the dissent from his favorite theory of invisible acoustic clouds in the atmosphere, expressed by Professor Henry in the Appendix to the Lighthouse Report of 1874, is sufficiently obvious. Indeed, its polemical tenor very clearly indicates that the question involved therein is simply one of explanation, — the acceptance or non-acceptance of certain views, — in no respect one of priority of observation of the fundamental facts. And, in fact, no anomaly of sound has been referred to, not embraced and discussed in the previous investigations of the U. S. Lighthouse Board, and no practical recommendations given for improving the fog-signal service with a view to the protection of life and of property at sea, not already adopted in this country.

After briefly reciting some of the instrumental experiments undertaken by the chairman of the U. S. Lighthouse Board, Professor Tyndall continues: "Further observations were made by Professor Henry and his colleagues in August, 1873, and in August and September, 1874. In the brief but interesting account of these experiments, a hypothetical element appears which is absent from the record of the earlier observations." (*Preface*, p. 13.) This tribute at least illustrates the caution of the true philosopher in framing hypotheses.

The key-note of Professor Tyndall's contribution to the theory of sound is atmospheric "flocculence," or non-homogeneity. On the remarkable variability in the range or penetration of sound observed on different days, he gives the following summary of his conclusions: "These discrepancies were proved to be due to a state of the air which bears the same relation to sound that cloudiness does to light. By streams of air differently heated, or saturated in different degrees with aqueous vapors, the atmosphere is rendered *flocculent* to sound. *Acoustic clouds* in fact are incessantly floating or flying through the air. They have nothing whatever to do with ordinary clouds, fogs, or haze. The most transparent atmosphere may be filled with them; converting days of extraordinary optical transparency into days of equally extraordinary acoustic opacity. . . . . Aerial echoes of extraordinary intensity and of long duration are thus produced. They occur, contrary to the opinion hitherto entertained, in the clearest air. . . . . The existence of these aerial echoes has been proved by observation and experiment. They may arise either from air-currents differently heated, or from air-currents differently saturated with vapor." (pp. 321, 322.)

This is simply an extension of the received doctrine of a non-homogeneous atmosphere being unfavorable to the transmission of sound; familiar for the last three quarters of a century, or since the observations of Humboldt on his tour in South America. Two years before Professor Tyndall's investigations, General Duane, in a Report to the U. S. Lighthouse Board, of observations made in 1871, had arrived at similar conclusions. This, however, was first published in the Report of the Lighthouse Board for 1874. The following are his remarks on the subject: "Now it frequently occurs that a signal, which under ordinary circumstances would be audible at the distance of fifteen miles, cannot be heard from a vessel at the distance of a single mile. This is probably due to the reflection mentioned by Humboldt. The temperature of the air over the land where the fog-signal is located being very different from that over the sea, the sound, in passing from the former to the latter, undergoes reflection at their surface of contact. The correctness of this view is rendered more probable by the fact that when the sound is thus impeded in the direction of the sea, it has been observed to be much stronger inland. . . . .

Experiments and observation lead to the conclusion that these anomalies in the penetration and direction of sound from fog-signals are to be attributed mainly to the want of uniformity in the surrounding atmosphere, and that snow, rain, fog, and the force and direction of the wind have much less influence than has generally been supposed." (*Lighthouse Report*, p. 104.)

Professor Tyndall follows his quotation of these passages with this comment: "The Report of General Duane is marked throughout by fidelity to facts, rare sagacity, and soberness of speculation. The last three of the paragraphs just quoted exhibit, in my opinion, the only approach to a true explanation of the phenomena which the Washington Report reveals." (*Preface*, p. 18.)

Now the rationale offered by Professor Henry, — the flexure of sound-rays by the unequal velocity of their wave-fronts, certainly embraces a larger variety of obscure and puzzling phenomena than that proposed by Professor Tyndall. The observed facts of sound being sometimes heard far beyond an intermediate region of inaudibility, — of its being heard through a given region of atmosphere in one direction, while wholly unheard in the reverse direction,— of its being heard at the mast-head of a vessel, after being lost on deck,— and even of its being occasionally heard with increased distinctness against the sensible wind, — are quite inexplicable on the "acoustic-cloud" hypothesis, while they receive a satisfactory explanation on the hypothesis of acoustic refraction.

Professor Henry, in publishing the Report of General Duane, thus comments on the passages above quoted: "In the foregoing, I differ entirely in opinion from General Duane, as to the cause of extinction of powerful sounds being due to the unequal density of the atmosphere. The velocity of sound is not at all affected by barometric pressure; but if the difference in pressure is caused by a difference in heat, or by the expansive power of vapor mingled with the air, a slight degree of obstruction of sounds may be observed. But this effect, we think, is entirely too minute to produce the results noted by General Duane and Dr. Tyndall, while we shall find in the action of the currents of wind above and below a true and sufficient cause." (*Lighthouse Report*, p. 104.)

Professor Tyndall quotes this paragraph (*Preface*, p. 18), and espouses very positively the opposite view of General Duane: "The observations at the South Foreland, where the sound has been proved to reach a distance of more than twelve miles against the wind, backed by decisive experiments, reduce to certainty the surmises of General Duane." (*Preface*, p. 19.) His presentation, however, of the view of his competitor, hardly exhibits his usual accuracy or courtesy. He says: "Professor Henry thus accounts for the fact that the north-east snow-wind renders the sound of Cape Elizabeth audible at Portland. In the higher regions of the atmosphere he places an ideal wind, blowing in a direction opposed to the real one, which *always* accompanies the latter, and which more than neutralizes its action. In speculating thus he bases himself on the reasoning of Professor Stokes, according to which a sound-wave moving against the wind is tilted upwards. The upper and opposing wind is invented for the purpose of tilting again the already lifted sound-wave downwards. Professor Henry does not explain how the sound-wave recrosses the hostile lower current, nor does he give any definite notion of the conditions under which it can be shown that it will reach the observer." (pp. 19, 20.)

This passage certainly does not indicate a very clear or correct comprehension of the doctrine of refraction as resulting from simple *differences* in the force or direction of the wind; and the critic has permitted himself to employ the word "is *invented*," notwithstanding that in the Lighthouse Report reasons are given, both from experiment and observation, fully justifying the assumption of an "upper opposing wind."

Professor Tyndall, with an air of "authority," proceeds: "This, so far as I know, is the only theoretic gleam cast by the Washington Report on the conflicting results which have hitherto rendered experiments on fog-signals so bewildering. I fear it is an *ignis fatuus* instead of a safe guiding light. Professor Henry, however, boldly applies the hypothesis in a variety of instances. But he dwells with particular emphasis upon a case of non-reciprocity which he considers absolutely fatal to my views regarding the flocculence of the atmosphere." (p. 20.)

Professor Henry may here be quoted at length: "That a flocculent condition of the atmosphere, due to the varying density produced by the mingling of aqueous vapor, is a true cause of obstruction in the transmission of sound, is a fact borne out by deduction from the principles of wave-motion, as well as by the experiments of the distinguished physicist of the Royal Institution of Great Britain; but from all the observations we have made on this subject, we are far from thinking that this is the efficient cause of the phenomena under consideration. A fatal objection, we think, to the truth of the hypothesis Professor Tyndall has advanced, is that the obstruction to the sound, whatever may be its nature, is not the same in different directions. We think we are warranted in asserting that in the cases of acoustic opacity which he has described, if he had simultaneously made observations in an opposite direction, he would have come to a different conclusion. That a flocculent condition of the atmosphere should slightly obstruct the sound, is not difficult to conceive; but that it should obstruct the ray in one direction and not in an opposite, or in a greater degree in one direction than in another, the stratum of air being the same in both cases, is at variance with any fact in nature with which we are acquainted. We would hesitate to speak so decidedly against the conclusions of Professor Tyndall, for whose clearness of conception of physical principles, skill in manipulation, and power of logical deduction, we entertain the highest appreciation, were the facts which were obtained in our investigations of a less explicit character." (*Lighthouse Report*, pp. 116, 117.)

Among several similar observations, the one mainly referred to is the case of a ten-inch steam-whistle at the Whitehead Light-station (about one mile and a half off the coast of Maine), suddenly ceasing to be heard by an approaching vessel within the distance of about three miles. Professor Henry notes that "the wind during this time was from the south, or approximately in an opposite direction to the sound"; a smaller steam-whistle on the vessel being meanwhile distinctly heard at the station. (*Lighthouse Report*, pp. 107, 108.)

The champion of "flocculence," still undaunted, replies: "But the atmosphere was not still, and a sufficient reason for the observed non-reciprocity is to be found in the recorded fact that the wind was blowing against the shore-signal, and in favor of the ship-signal." (*Preface*, p. 21.) How this favors the acoustic-cloud hypothesis is not very apparent. Professor Henry had previously remarked: "At first sight, the explanation of this might seem to be very simple, namely, that the sound is borne on in the one direction and retarded in the other by the motion of the wind. But this explanation, satisfactory as it might appear, cannot be true. Sound moves at the rate of about 780 miles an hour, and therefore, on the above supposition, a wind of 7.8 miles per hour could neither retard nor accelerate its velocity more than one per cent,—an amount inappreciable to ordinary observation; whereas we know that a wind of the velocity we have mentioned is frequently accompanied with a reduction of the penetrating power of sound of more than fifty per cent." (*Lighthouse Report*, p. 106.)

Professor Tyndall continues: "But the argument of Professor Henry, on which he places his main reliance, would be untenable, *even* had the air been still. By the very aerial reflection which he practically ignores, reciprocity may be destroyed in a calm atmosphere. In proof of this assertion, I would refer him to a short paper on 'Acoustic Reversibility' printed at the end of this volume." (*Preface*, p. 21.) The condition, "*even* had the air been still," is certainly curious, in view of the fact that "the argument of Professor Henry" is based directly on the existence of differences in the velocity of the wind!

The chapter on "Acoustic Reversibility" so confidingly referred to, forming "Appendix II.," and the conclusion of the work (pp. 403 – 411), is probably the least satisfactory essay in the volume. The card-board screen experiment, permitting sound to be diffracted around the edge of the card to a nearer point in one direction than in the reverse direction, appears quite foreign to the remarkable case of "non-reversibility" observed by Arago in 1822; and on no known physical principle can it be shown that a flocculent veil in the atmosphere will be "diacoustic" on one side, and "catacoustic" on the opposite side. It is the conception, of a transparent mirror, with a valve so cunningly arranged, that all the rays falling on one side will pass through without obstruction, while all the rays falling on the other side will be reflected back.

And yet, with a complacency that almost seems infatuation, Professor Tyndall continues: "The most remarkable case of non-reciprocity on record, and which, prior to the demonstration of the existence and power of acoustic clouds, remained an insoluble enigma, is there shown to be capable of satisfactory solution. These clouds explain perfectly the 'abnormal phenomena' of Professor Henry. Aware of their existence, the falling off and subsequent recovery of a signal-sound, as noticed by him and General Duane, is no more a mystery than the interception of the solar light by a common cloud, and its restoration after the cloud has moved or melted away. The clew to all the difficulties and anomalies of this question is to be found in the aerial echoes, the significance of which has been overlooked by General Duane, and misinterpreted by Professor Henry." (*Preface*, p. 21.)

It is believed that the real cause of the abnormal phenomena noticed lies much deeper than is here supposed; and that to every unbiassed physicist it can be satisfactorily shown that the true solution of the enigma has been discovered by Professor Henry, and has been as signally missed by Professor Tyndall; * nay, that it has even failed to be rightly understood by him, after having been propounded.

---

* This subject has been quite recently discussed, in the American Jour. Science for January and February.

"If we accept for a moment," says he, "a statement which Professor Henry seems to indorse, that sound-waves of great intensity, when they impinge upon a solid or liquid surface, do not obey the law of incidence and reflection, but 'roll along the surface like a cloud of smoke,' it only increases the difficulty." (*Preface*, p. 23.) There was no occasion for attributing this inexact comparison to Professor Henry. The phrase quoted is from General Duane, who says in his Report: "Upon a large scale, sound does not appear, on striking a surface, to be reflected after the manner of light and heat, but to roll along it like a cloud of smoke." The only "seeming indorsement" in Professor Henry's comment is: "This statement is in a measure in accordance with results which I have previously found," that "powerful sounds tend to diverge laterally to such a degree as to render reflectors of comparatively little use." (*Lighthouse Report*, p. 100.) The statement of Professor Henry is simply one of fact, ascertained by very decisive experiments, the details of which are given in his Report. Nor is this so surprising as might appear, if we consider that the dimensions of a sound-reflector cannot conveniently exceed an average wave-length of sound. Were it possible to construct such a reflector, comparable in this respect to even the very smallest ever employed optically, we should probably have that distinct relation of incidence and reflection observed in light, and proportionately in ordinary sounds within small distances.

If we may judge from Professor Tyndall's earnest invective (rather oddly interpolated into his "aerial echoes"), he has at last triumphantly burst asunder the shackles of "authority" which had previously bound him, and now stands forth disenthralled and exultant.

"The clew to all the difficulties and anomalies of this question is to be found in the aerial echoes, the significance of which has been overlooked by General Duane, and misinterpreted by Professor Henry. And here a word might be said with regard to the injurious influence still exercised by authority in science. The affirmations of the highest authorities, that from clear air no sensible echo ever comes, were so distinct that my mind for a time refused to entertain the idea. Authority caused me for weeks to depart from the truth, and to seek counsel among delusions.* On the day our observations

---

*** On page 258 occurs this remarkable statement: "The publication of Dr. Derham's celebrated paper in the Philosophical Transactions for 1708 marks the latest systematic inquiry into the causes which affect the *intensity* of sound in the atmosphere." This systematic inquiry does not however appear to have been read; for "the affirmation of *this* authority" (however "injurious its influence") is certainly "distinct,"— one hundred and sixty-seven years ago. "I will add also an**

at the South Foreland began, I heard the echoes. They perplexed me. I heard them again and again, and listened to the explanations offered by some ingenious persons at the Foreland. They were an 'ocean-echo.' This is the very phraseology now used by Professor Henry ; . . . . one of the phases of thought then passed through, one of the solutions then weighed in the balance and found wanting, being identical with that which Professor Henry now offers for acceptation. But though it thus deflected me from the proper track, shall I say that authority in science is injurious? Not without some qualification. It is not only injurious, but deadly, when it cows the intellect into fear of questioning it." (*Preface*, p. 22.) To what is all this *apropos?*

This is the very style of protest one is accustomed to hear from some automatic-motor inventor, quadrature discoverer, anti-Newtonian philosopher, or transcendental mathematician intent on showing ("in the absence of all proof to the contrary") that a straight line indefinitely prolonged may possibly cease to be rectilinear. But surely such fervid "air-beating" is scarcely worthy of one who has himself earned the right (perhaps not altogether unconsciously) to speak with some authority on certain topics of science. It is not usually the intelligent and genuine seeker after simple *truth* who feels any occasion for emancipation from the trammels of "*authority* in science." The only thing approaching such sway recognized by the cultured is the deference justly paid to the opinion of experts, in the range of their special explorations. Science has no hierarchy, and hence no dogma. Its domain is absolutely republican.

But when Professor Tyndall declares that "authority" caused him to *depart* from the truth, and that "the affirmations of the highest authorities" were "distinct," that from clear air no sensible echo *ever* comes, it is to be feared that he has been the unconscious victim of a non-scientific use of the imagination. It may be doubted whether such "distinct affirmations" can be found in any authority ("highest" or lowest) on the subject of sound. We should as much expect to find the distinct affirmation by an expert in optics, that from the empty ether no luminous reflection ever comes, — a proposition which would yet probably so command the unhesitating assent of Professor Tyndall, that "his mind would refuse to entertain the idea" of its denial. The truth is that the human mind is fortunately

---

example of *the reflection of sound in the air from aerial particles.* When I have heard the boom of great guns of war, especially in a clear and tranquil sky, I have quite frequently observed a murmur aloft in the air, preceding the report."—*Phil. Trans.* 1708, XXVI. 2.

so constituted, that it hesitates to adopt a new apprehension not clearly justified by conscious experience; and Professor Tyndall "for a time refused to entertain the idea" of aerial echoes, doubtless for the same reason that some of his readers will continue to do so, *notwithstanding* his "authority."

Our author continues: "But the authority which so merits our respect as to compel us to test and overthrow all its supports, before accepting a conclusion opposed to it, is not wholly noxious. On the contrary, the disciplines it imposes may be in the highest degree salutary, though they may end, as in the present case, in the ruin of authority." (*Preface*, p. 22.) Authority in science is injurious; but with this qualification it may be "deadly," or it may be only partially "noxious." An impassive reader may well wonder what fortress of scientific pretension it is which (though "meriting respect") has been so shattered and demolished.

Had some enthusiastic believer in the "authority" of Professor Tyndall proclaimed for his hero, that having invested the fastnesses of "the highest authorities" preceding him, — by the salutary discipline acquired in "testing and overthrowing all the supports" of the besieged, — he had finally achieved a victory ending in the total "ruin of authority," the bulletin would probably be accounted by the judicious as both fulsome and inane. As the utterance of the intellectual Luther himself, it may be left without characterization.

Not the least characteristic feature exhibited throughout this controversial "Preface" is the lofty complacency exhibited by its writer. For example: "With regard to the particular notion which now finds favor with Professor Henry, it suggests the thought that his observations, notwithstanding their apparent variety and extent, were really limited as regards the weather. For did they, like ours, embrace weather of all kinds, it is not likely that he would have ascribed to the sea-waves an action which often reaches its maximum intensity when waves are entirely absent." (*Preface*, p. 23.) The suggestion that Professor Henry probably made observations only during stormy weather, sounds not a little whimsical in reference to investigations on this subject, scattered over ten years, and along hundreds of miles of sea-coast.

Or once more: "Rightly interpreted and followed out, these aerial echoes lead to a solution which penetrates and reconciles the phenomena from beginning to end. On this point I would stake the issue of the whole inquiry, and to this point I would, with special earnestness, direct the attention of the Lighthouse Board of Washington. Let them prolong their observations into calm weather. . . . . The echoes afford the easiest access to the core of this question, and it is for this reason that I dwell upon them thus emphatically. It requires no refined skill or profound knowledge to master the conditions of their production; and, these once mastered, the Lighthouse Board of Washington will find themselves in the real current of the phenomena, outside of which — I say it with respect — they are now vainly speculating." (*Preface*, p. 24.)

And this attitude of condescending dignity is assumed toward one whom he had recognized while in America as our "scientific Nestor," and had extolled as being already "in the van of experimental inquiry," when the banqueted speaker was yet a novice in science.* His placid confidence in his own cherished hypothesis never permits a suspicion that *it* may possibly lie "outside the real current of the phenomena"; and on his self-consciousness no imagination dawns that the rival hypothesis, so magisterially dismissed by him as an *ignis fatuus*, is really a theory, based upon a series of observed facts, and fertile in suggesting explanations of a range of seemingly incongruous effects, otherwise quite beyond the reach of any existing speculation.

So completely does Professor Tyndall seem to himself to have grasped the subject and exhausted its product, that he concludes: "if Professor Henry will but adopt the same views regarding aerial echoes, the acoustic deportment of the atmosphere in haze, fog, sleet, snow, rain, and hail will be no longer a mystery: even those 'abnormal phenomena,' which are now referred to an imaginary cause, or reserved for future investigation, will be found to fall naturally into place, as illustrations of a principle as simple as it is universal." (*Preface*, pp. 24, 25.)

Professor Tyndall thus epitomizes his labors: "In the years 1873 and 1874 this subject received an exhaustive examination, observational and experimental." (p. 321.) *Jamque opus exegi.* — "Thus ends for the present, at all events, an inquiry which I trust will prove of some importance, scientific as well as practical." (p. 319.)

Professor Henry, whose researches on the subject have been much more extensive, — while the Trinity House "has established no facts of practical importance which had not previously been observed and used by the Lighthouse Board," and who has really made a contribution to our knowledge, of far wider reach and higher value, — in the concluding paragraph of his Report speaks thus: "Much further investigation is required to enable us to fully understand the effects of winds on the obstruction of sound, and to determine the measure of the effect of variations of density in the air, due to inequality of heat and moisture. But such investigations can only be made under peculiar conditions of weather and favorable localities, with the aid of a number of steamers, and a series of observers, by whom the transmissibility of the air may be simultaneously observed in different directions. . . . . It is the intention of the Lighthouse Board to continue observations in regard to this matter, and to embrace every favorable opportunity for their prosecution under new and varied conditions." (*Lighthouse Report*, p. 117.)

---

* **Lectures on Light: in U. S. — Am. Ed., 1873 (pp. 10, and 1861).**

Professor Tyndall has failed to realize the practical importance of acoustic refraction; which is unquestionably a real phenomenon, and which may result from two independent causes or conditions. First, there is the refraction occasioned by differences of aerial currents, suggested by Professor Stokes, in 1857; and, second, the refraction occasioned by differences of aerial temperature, pointed out by Professor Reynolds, in 1874. While he is of course fully aware that the velocity of sound is affected by differences of wind, as well as by differences of temperature, he is not aware that an easy calculation will show the resultant deflection to be in many cases quite considerable. Turning to Section 6 of Chapter I., which treats of the "Refraction of Sound," the only illustration of the subject we find is a reference to the experiment of Sondhauss (now nearly a quarter of a century old), showing the convergence of sound to a focus by the use of a carbonic-acid gas lens in a thin membranous envelope." (pp. 19, 20.) This exhausts the subject.

And yet the observations of Professor Tyndall himself furnish evidence on this point, the true significance of which has entirely escaped him. In his experiments at the South Foreland, there were at first employed on the beach, about forty feet above high-water mark, two fog-horns, each eleven feet long, and an air-whistle with six-inch cups; and on the top of the Dover cliffs, about two hundred feet above these, another set of instruments of precisely the same size and power. "Numerous comparative experiments made at the outset gave a slight advantage to the upper instruments. They therefore were for the most part employed throughout the subsequent inquiry." (*Sound*, p. 263.)

Is it not strange that Professor Tyndall's inquisitive mind neglected to search into the meaning of this "slight advantage"? Or was he at once satisfied that flocculent clouds had something to do with it? Here was a difference, — not accidental, — but confirmed by "*numerous* comparative experiments," and in a direction, too, contrary to that which might have been anticipated; for not only were the lower instruments in denser air, and therefore theoretically in a condition to give a louder sound with the same motive-power, but by being near to the resisting and protecting wall of the sea-surface, were favorably placed for distinctly transmitting their sound to a distance: added to which was the extended wall of chalk-cliff, two hundred feet high, immediately behind the instruments, for reinforcing and reflecting the sound-waves seaward.

What, then, is the true explanation of this apparent anomaly? That the stratum of air two hundred feet above the sea was ordinarily (or when still) cooler than that near the surface, is hardly doubtful. Professor Tyndall has stated for us the well-known fact, that "inasmuch as the air above is colder than that below, the actual velocity on the summits of mountains is less than that at the sea-level." (*Sound*, p. 23.) The difference being, at ordinary temperatures, about one foot per second for each degree Fahrenheit. Here, then, with the admitted fact of the lower portion of the sound-wave moving more swiftly than its upper portion, we have the inevitable condition of a flexure of the horizontal rays of sound upward as they advance; and however slight this refraction, as it is continuous or cumulative, in the course of several miles it must make itself felt. Obviously, the limiting point of tangency with the sea, of the upward bent rays of a given curvature, must be much nearer to the lower source of sound than to the upper; and hence while at the distance of ten miles the curved rays of sound from the top of the cliff might reach the deck of a vessel, those from its base would pass considerably above the vessel.

Whenever a sea-breeze should blow toward the fog-signal, as it would commonly be stronger above, and more resisted and lagging below, the refraction would generally be reinforced by the still greater retardation of the upper part of the sound-waves than the lower part. On those rare occasions where neither of these conditions should exist, that is, neither an unequally retarding wind nor a diminished temperature in the upper strata, there can be no doubt that then the lower instruments would prove the more effective.

Allusion has already been made to the "conspicuous absence" of any research into the prominent and familiar fact of the influence of *wind* on sound-range. Dr. Robinson, in his Report on "Fog Signals" in 1863, remarked that "a gun or bell which is heard several miles *down* the wind, is inaudible at more than a few furlongs *up* it." (*Rep. Brit. Assoc.* 1863, XXXIII. 106.) And Professor Tyndall gives his distinct testimony to the fact, that "it is within the mark to say that the gun on October 11th was heard five times, and might have been heard fifteen times as far to leeward as to windward." (*Sound*, p. 309.) Strangely enough, a result so remarkable and so well determined by repeated observation, receives from him no elucidation, and is left by him without even a suggestion toward interpretation! What conceivable condition or arrangement of "flocculence" — that persistent obscurer — could be supposed to have its transparency or opacity to sound multiplied so many times by a simple difference of direction in the wind?

Another instance may be cited of our author's lack of comprehensive insight into the full import of his own observations. He says: "Up to July 3d, all remained enigmatical; but on this date observations were made which seemed to me to displace surmise and perplexity by the clearer light of physical demonstration. . . . . At 2.15 P. M., and at a distance of three and three fourth miles from the station, with calm, clear air and a smooth sea, the horns and whistle were sounded, but they were inaudible. Surprised at this result, I signalled for the guns. They were all fired, but though the smoke seemed at hand, no sound whatever reached us. . . . . We steamed on in the same bearing to two miles, and had the guns fired point blank at us. The howitzer and the mortar, with three-pound charges, yielded a feeble thud, while the eighteen-pounder was wholly unheard." (pp. 268, 269.) "As I stood upon the deck of the *Irene*, pondering the question, I became conscious of the exceeding power of the sun beating against my back and heating the objects near me. Beams of equal power were falling on the sea, and must have produced copious evaporation." (p. 271.) Hence atmospheric "flocculence," and as a result, acoustic opacity. "Curiously enough, the conditions necessary for the testing of this explanation immediately set in. At 3.15 P. M., a solitary cloud threw itself athwart the sun and shaded the entire space between us and the South Foreland. The heating of the water, and the production of vapor, — and air-currents were checked by the interposition of this screen. . . . . The sounds, as I expected, were distinctly, though faintly heard. This was at three miles' distance. . . . . We pushed the test further by steaming farther out. At five and three fourth miles we halted and heard the sounds; at six miles we heard them distinctly." (pp. 272, 273.)

That this interesting experiment furnished a satisfactory test of something more than aerial "flocculence" does not appear to have entered the observer's mind. So preoccupied was his mental vision with a *hypothetical* image, that it had no perception of that *vera causa*, the necessary refraction of the acoustic rays under the conditions stated, — the lifting of the waves of sound above the listener's head through the heated atmosphere of the July sun; and the subsequent flattening or unbending of these rays as the lower air grew cooler. That the powerful and penetrating sounds of steam-whistles, fog-trumpets, and cannon — having an easy range of a dozen miles — could be absolutely *extinguished* by vapor-clouds at the distance of two miles appears quite incredible. That such sounds might be entirely deflected away, and so become inaudible in a given direction without losing their range, is entirely conformable to what we know. And that the larger amount of vapor in the lower strata of air should also increase this refraction, by rendering the air more absorbent of heat, is no less accordant with the indications of theory.

Professor Reynolds, to whom belongs the credit of this fine exposition, well remarks: "Here we see that the very conditions which actually diminished the [sensible] range of the sound were precisely those which would cause the greatest lifting of the waves. And it may be noticed that these facts were observed and recorded by Professor Tyndall, with his mind altogether unbiased with any thought of establishing this hypothesis. He was looking for an explanation in quite another direction. Had it not been so, he would probably have ascended the mast, and thus found whether or not the sound was all the time passing over his head. On the worst day, an ascent of thirty feet should have extended the range nearly a quarter of a mile." (*Proceedings R. S.* 1874.)

To conclude this somewhat extended review, the judgment must be pronounced that the additional matter introduced into this new or third edition of "Sound" occupies a space very considerably in excess of its real importance. Had the ill-advised preface been suppressed, and the prolix Chapter VII. been condensed into something like its present "summary," the just proportions of the treatise would have been much better maintained. If, in addition to this, the section on "Refraction of Sound" had been extended and enriched with the pregnant results of the researches of Professor Henry and of Professor Reynolds, the value of the work as an exposition of the present state of the science of acoustics would have been greatly enhanced.